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SHORTER ARTICLES AND DISCUSSION

TRICHOMONAS AND BLACKHEAD IN TURKEYS

IN reading the introduction to Dr. E. E. Tyzzer's contribution in the May issue of the *Journal of Medical Research* entitled "Developmental Stages of the Protozoon of 'Blackhead' in Turkeys," one is almost certain to be left with the impression that the conception of the agency of the common flagellate, *Trichomonas*, in producing pathological conditions characteristic of blackhead in turkeys, as described in several papers by the present writer, has no legs to go on, and would scarcely receive the consideration of sane protozoologists. Of course this is not the impression that Dr. Tyzzer meant to leave; so that it is fortunate that, in the experimental section of the paper referred to, he makes certain observations which are more favorable to the "flagellate hypothesis." Fearing, however, lest the hypothesis of tissue-invasion by *Trichomonas* might as yet be too frail to survive long under the criticism of two such men as Dr. Tyzzer and Dr. Theobald Smith (formerly chief proponent of the "Amebic theory"), the present writer, who first had the misfortune seriously to mention *Trichomonas* in connection with blackhead, wishes to point out a few instances in which Dr. Tyzzer's criticisms, real or implied, are due either to careless reading of the original papers, or to a too hurried examination of the plates, or to both:

In way of introduction it may be said that Dr. Smith's first exposition of the blackhead disease, together with his original description of the causative agent, *Ameba meleagridis*, appeared in 1895. It is true that, at that time, as Dr. Tyzzer states, the possibility of the relationship between *Ameba meleagridis* and the flagellates was suggested by Dr. Smith. And the suggestion was expressed in these words:

There is probably no genetic relation between this hypothetical organism (flagellate) and the true parasite of the disease under consideration.

For twenty years the "Ameba hypothesis" stood; and it was not until this interpretation was called into question by Cole and the present writer that (as Dr. Tyzzer states), Dr. Smith explained "that the name 'Ameba' is employed tentatively, and

that it may be necessary to change this when the nature of the parasite is better understood." The present writer would probably have been more cautious in his original criticisms of Dr. Smith's conclusions if that statement had been made by Dr. Smith in 1895 instead of 1915. This will answer a criticism of Dr. Tyzzer's that is rather subtle and only implied.

Again, it is implied that something must be wrong with an investigation that purports to demonstrate that *Trichomonas* is the causative agent in an infection, when the investigator can not put his finger on the species concerned, or even risk the foundation of a new species. Who will come forward and give us a clear, definite and usable classification of the Trichomonads! And, speaking of new species in such a poorly known group, one can not help wondering if it would not have been just as well, in an earlier case, to leave the "*meleagridis*" off of *Ameba*. Can one doubt that many unhappy hours and profitless discussions have resulted from the necessity of piling up premature adjectives after an innocent Latin noun? Why embarrass the lexicographers until we are sure? And in the case under consideration the present writer wasn't sure.

Again Dr. Tyzzer states that it is obvious that the present writer "does not consider the organism as primarily pathogenic in nature, but as a normal inhabitant of the alimentary tract of turkeys and fowls which may invade the tissue under conditions which lower the resistance of the host." This is quite true. Then Dr. Tyzzer continues,

Apparently this author attaches no importance to the fact that the disease may be produced in healthy flocks by the introduction of infected birds.

This is also quite true. The writer does not know of a carefully controlled experiment in which it has ever been conclusively demonstrated that blackhead has been produced in healthy flocks by the introduction of infected birds. During one year of experimental work in the field the writer made it a point of removing the ceca and livers of poults which died of blackhead, chopping them up in a meat cutter, mixing lightly with middlings and feeding *en masse* to other poults as a partial substitute for beef-scraps. The mortality from blackhead in the fed group and in the control group was essentially the same. Most of the poults died after several weeks with gape-worm infection, and with no sign of pathologic changes in either ceca or liver. The

writer would have no apprehension in feeding to "healthy" poultz any reasonable amount of pathologic material from black-head cases, *provided it were done in such a way as not to upset the normal digestive equilibrium, and not to introduce pathogenic bacteria nor bacterial toxins.*

If Dr. Tyzzer had seen as much of blackhead in the field and on the farm as he has seen in the laboratory, he might more readily find reason in the writer's viewpoint. Until a few years ago, the writer held strongly the same views which Dr. Tyzzer now holds. But here, as in some other branches of science, "field work" and field experience has often wholesomely corrected misguided laboratory theory; at least the writer has found it so in his own case.

In another place Dr. Tyzzer states:

Contrary to Hadley's claim *Amœba meleagridis* should not be regarded as a cell parasite. . . . It does not occur within cells except after motility is lost, when it is soon phagocyted.

Regarding the matter of cell-invasion Dr. Tyzzer quotes from a passage from the author but stops prematurely. The passage should be read as a whole to obtain the writers' full meaning. The writer states that, in tissue-invasion, we see *Trichomonas* in a new rôle, and that here it may actively invade living cells. At this point Dr. Tyzzer's quotation stops, but in the original the text proceeds:

One may remark that the type of cell invaded is a highly specialized type [endothelial], and one that, by its nature, is more or less open to invasion.

The writer points out elsewhere that this invasion is not passive but active. But nowhere in any of his published papers (except in reference to the "goblet" cells) does the writer give any expression of the opinion that *Trichomonas* is a cellular parasite in the same sense that applies to the coccidia or other sporozoa. In this respect, Dr. Tyzzer accidentally misrepresents the writer's views. Such little mistakes as always likely to happen in the hurried reading of long and complicated papers.

A further criticism of Dr. Tyzzer's is too good to omit. The circumstances are as follows: The epithelium of the cecum of the turkey is thrown into folds. Sometimes they are deep and sometimes shallow. Within the folds, next to the cecal wall, are the crypts. The projecting folds, with their accompanying tissues,

the writer has referred to as the villi. The author points out that invasion of the submucosa is brought about by the passage of the flagellates through the epithelium of the fundus of the crypt, and that secondarily they invade (from behind) the villi, and finally escape into the cecal lumen after pushing off the epithelium of the villus tips. This phenomenon, which can be followed clearly in suitable sections, the writer has referred to as the stage of "reversed infection," and has pointed out that it constitutes a means whereby the parasites complete their parasitic cycle, rather than being buried and destroyed within the tissues as stated by Dr. Smith, who is of the opinion that the parasite of blackhead lacks this essential feature of perfect parasitism.

Here is Dr. Tyzzer's criticism of this exposition:

The fallacy of such reasoning is quite apparent when the facts of the case are considered. There are no villi in the portion of the cecum commonly involved in blackhead.

The writer had carefully explained in the text the appearance of the invaded tissues; he had pictured it by hand-drawings, and more in detail by a series of photomicrographs. No one could fail to understand the definite histological structure to which the writer referred, whether it is properly termed a "villus," or something else. Dr. Tyzzer may call the histological structure what he pleases. The facts of the case with reference to *Trichomonas* remain the same.

But leaving aside the propriety of the term, villus, let us consider what Dr. Tyzzer means by the balance of his sentence ". . . in the portion of the cecum commonly involved in blackhead." In the examination of hundreds of cases of blackhead in turkeys and wild fowl the writer has found that blackhead lesions may be initiated anywhere in the cecal wall; there is no part of the cecum that is "commonly involved" except for this circumstance: the majority of the lesions are observed in the distal half of the cecum. Thus Dr. Tyzzer neglects clearly reported facts to grapple with a technical triviality in nomenclature; and at the same time, manifestly from lack of experience with many cases of the disease, misrepresents one of the essential facts relating to cecal infection.

In the next sentence Dr. Tyzzer attacks the statements of the writer regarding the avenue of infection of *Trichomonas*. Re-

ferring to the separation of the epithelium from the basement membrane, he states:

In one case the separation of the epithelium is taken as evidence of invasion, and in the other it is taken as evidence of escape of the flagellates from the tissue.

Dr. Tyzzer quite mistakes the point involved. It is not the separation of the epithelium that is the important point (since this is often an artifact), but the orientation and grouping of the parasites in the vicinity of this epithelium. By looking at a church door we can scarcely tell whether the last congregation went in or out, but if we can find the congregation the question will probably be answered.

And in further criticism of this point (avenue of infection and of exit) Dr. Tyzzer has the misfortune to state,

The organisms interpreted by Hadley as encysted forms of the flagellate being discharged from the tissue are evidently *Blastocystis* derived from the cecal content.

Did Dr. Tyzzer fail to examine the writer's photomicrographs (Bulletin 168, Figs. 30, 32 and 36) together with the complete description of these figures on a preceding page? Did he fail to read the description of this "reversed infection" on page 26? Are the writer's photographs so poor as to make possible a confusion between a flagellate trophozoite and "*Blastocystis*," or has Dr. Tyzzer an inadequate conception of what *Blastocystis* really looks like? And, in addition, may it not be a little inaccurate to affirm that "there is now quite general agreement that they (*Blastocystis*) represent a distinct type of organism . . ."? The matter is apparently still in controversy.

As to the statement of Dr. Tyzzer that the writer has failed to establish the identity of the parasite with any species of *Trichomonas*, or "to demonstrate any features characteristic of the genus,"—this must be left for others to judge. But the author can not forbear to reiterate that he has no reason to withdraw the evidence presented in previous papers. The strongest evidence of all comes from the relatively rare cases in which one can trace from the beginning the movements of the parasites in the tissues, and follow clearly the morphological changes that they undergo as the infection proceeds. It would seem that Dr. Tyzzer, in his examination of only "five infected turkeys," has never seen such cases. The present writer worked for many years before he found the ideal specimens. It is a

hard thing to realize, in such an investigation where one is attempting to ascertain the relation of two widely different entities, that a single average case, even though admirably sectioned and stained, may mean very little. Dozens of cases usually afford a more comprehensive view; and finally one comes to be able to piece together bits of information which make the story clear. It would be miraculous if the keenest pathologist could make clear the evidence from "five cases." Protozoan life histories are not read in a moment, and a study of a hundred cases for an hour means much more than one case for a hundred hours,—unless that one case is exceptional.

In concluding, it may be added that the writer hopes later to consider more in detail the valuable *constructive* aspect of Dr. Tyzzer's paper. It is freely admitted that the life history of *Trichomonas* in the tissues is not wholly clear, and it seems possible that some of the forms referred to by Dr. Tyzzer are new. This is especially true of some of the motile stages which, in the tissues, lose their flagella and, as Schaudinn says, "auch mit stumpfflobosen Pseudopodien umherkreicht." It will probably be some years before the last word is said on the blackhead problem; and yet we are progressing. Under an efficient smoke screen Dr. Tyzzer has given the last blow to the "Amebic theory" and already—though grudgingly—has yielded some support the agency of the flagellates in cecal and liver infections. It may be confidently expected that in the course of time his researches will give more.

Dr. Tyzzer closes his critical introduction with the following words: "It may appear that the above discussion is unduly critical of the findings of other investigators. The confused state of the subject, however, appears to warrant drastic methods and the singling out of various misinterpretations and inconsistencies, for it is quite evident that the enthusiasm of certain investigators for their views has caused them to neglect important facts."

How we all wish to be such champions of the truth! But, in our war on "misinterpretations" and "inconsistencies" and on "neglect of important facts," would not our scientific world be a happier place, and all our work of greater merit, if criticism were tempered more with keen insight and less with the ardent spirit of academic chivalry?

PHILIP HADLEY.

KINGSTON, R. I.,

October 9, 1919